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Hydrology and Earth System Sciences Discussions

Interactive comment on "Temporal variations of evapotranspiration: reconstruction using instantaneous satellite measurements in the thermal infra red domain" *by* E. Delogu et al.

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Specific comments: 1) Organization of the paper is not good and clear: - Introduction is generally too long, and can be made more concise.

This is also suggested by all other reviewers; the paper will be reorganized and clarified as suggested.

For example, some results already provided in Introduction section, (e.g. pp 1704, lines 3-5: "This work also : : :". The results of the paper should be presented only under Results section, not in the introduction.

"This work also. . ." refers to the work by Hoedges et al. (2008), not the present paper, this will be clarified. It will be emphasized also that this previous work is on one dataset only.

- Methods are not presented in an organized way. Section 2.1 is called "Method", but in sub-sections 2.1.1 and 2.1.2, some background theory is presented related with the two methods. The real methodology description (of the paper) starts with section 2.3. Therefore, section 2.1 could be named as "Background theory" and all the sections starting with 2.3 could be grouped under a "method description" section as sub-sections.

This is an excellent suggestion which will be taken into account.

- Additionally, providing a flowchart for the two alternative ways (tested in the paper) would improve the readability of the paper.

A flowchart will be added to show the different steps of both methods.

2) Terminology used in the paper (especially some equations and variable names) is confusing:

- In Method 2.1.1 (EF method) and 2.1.2 (SF method), there is confusion between Eq. 1 (pp 1707) and Eq. 3 (pp 1708). In Eq. 1, ETd denotes daily evapotranspiration. But in Eq. 3, what does LETd denote is not mentioned? Is it daily evapotranspiration, or daily total latent heat flux? Since the aim of the paper is to assess the performance of two methods (EF and SF), what they calculate should be consistent and comparable (either both daily evapotranspiration, or both latent heat).

What is compared is indeed daily evapotranspiration (in mm) for both methods. The use of the terminologies will be made consistent and the units of the variables will be clearly provided.

- Besides Eq. 1 and Eq.3, especially some abbreviations (such as LET) are misleading/ confusing sometimes. LE generally denotes "Latent heat – the energy consumed

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for evapotranspiration in W m-2" and ET denotes evapotranspiration (in mm d-1, mm h-1, etc.). I suggest sticking to this generally accepted terminology instead of introducing variable names like LETd, LETp, LETpd. Instead of LETp, ETp could be used to represent potential ET (instantaneous) and ETp-d for daily potential ET.

The terminology will be unified and all daily fluxes expressed in mm/d and all hourly fluxes in mm/h.

- In most of the equations, the units of the variables are not explained. The units should be clearly provided under each equation.

3) About Results: - In section 3.1.1, 10% overestimation is mentioned for estimated AEd (compared to measured), which is corrected using 0.9 factor (Eq. 6). However the result of this comparison is not shown. Considering its huge effect on the ET results (in pp 1717 lines 3-7, it is mentioned that all the error statistics related with ET estimations were improved when Eq. 6 was used instead of Eq. 4), it can be good to provide a graph of AEd comparison (estimated vs. observed) because applying a constant factor means there is a systematic bias between estimated and observed AEd.

The following additional figure presenting data from all sites will be provided to support our findings.

- There is a wrong calculation of the average % error related with the Table 5. In pp 1722 line 18, the average error for improved EF (Eq.7) is given as 1.9%. However, this average calculation didn't pay attention to the cancelling-out effect of error signs (for example, average of 3.6% and -2.1% is not 0.8%, but it is 2.9%). Based on the errors given in Table 5, the real average error for variable EF is found as 6.5%, not 1.9%.

This will be corrected in a revised version.

- In section 3.3. (pp1722, lines 17-19), the authors indicate the average errors of 15.8% for constant EF and 1.9% (in reality 6.5%) for variable EF. As understood from section 2.2., measurements from eddy covariance systems were used as the reference for

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these comparisons. Kalma et al. (2008 and references therein) provides an extensive review of land surface evaporation estimation, and state that even the ground-based ET flux measurements derived from Bowen ratio and eddy covariance systems have an uncertainty of around 20-30% generally. However, the authors don't mention about these in-situ measurement uncertainties in the results section. With the current in-situ measurement uncertainty levels, is it really realistic to achieve EF estimation accuracies as low as 1.9%?

We agree with the need to state more clearly the uncertainty associated with what we call "reference", and comment on it. However, the large reduction in error for all sites but one gives us some confidence on the validity of the bias reduction.

Additionally, it is understood that the proposed variable EF (Eq.7) provides improvement only for little or no water stress days, while constant EF gives better result for water stress days. If not generally applicable, can the proposed Eq.7 be really an "efficient operational" alternative? A better discussion of the results is needed in section 3.3.

For operational applications, it is difficult to implement different algorithms depending on hypothetical stress levels (stress is actually the target variable of most TIR based evapotranspiration estimates). It is more important, for robustness considerations, to ensure that an additional algorithm does not deteriorate the performance, which is the case in our study for all sites but one (and for this one the deterioration is really minimum compared to the large improvement of the performance for all other sites).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1699, 2012.

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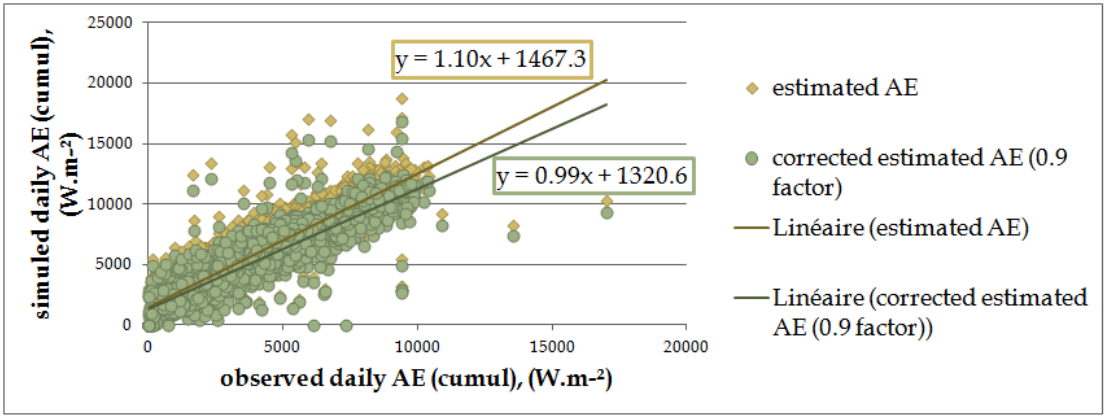

Fig. 1.

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